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Correspondence

PSYCHIATRIC OUT-PATIENTS IN PLYMOUTH

DEAR SIR,

The reply of Drs. Kessel and Hassall (May, 1965, p. 449) impels me to go into greater detail concerning their paper on psychiatric out-patient services.

My criticism is that their analysis of psychiatric out-patient services in a general hospital was undertaken in an area where the psychiatrists were based on a large mental hospital and the authors derived a general conclusion from this analysis (viz. that "general physicians and surgeons do not refer most of the psychiatric patients they recognize") without qualifying it in any way by referring to, and taking account of, the specific setting in which the investigation was carried out. I would have had no objection to the conclusion if, say, it had read, "In an area where the psychiatric out-patient clinics held in a city general hospital are staffed by psychiatrists based on a large mental hospital situated thirteen miles from the city centre, general physicians and surgeons do not refer most of the psychiatric patients they recognize." In a paper concerned with providing information for the future development of psychiatric out-patient services and the effect of such services on patient demand for care, the question of whether the psychiatrists are based primarily on a mental hospital or a general hospital is of fundamental importance and has a direct bearing on the problems under consideration. The omission of any discussion of this aspect of the subject detracts, in my view, from the significance of the findings.

I would have thought it obvious, from the context, that the references I gave in my letter were cited solely to enable the reader to gain some idea of the type of comprehensive psychiatric unit that I had in mind, and not as sources for out-patient statistics. In fact, I stated that I had not got figures relating to sources of referral readily available. Although, however, I have not got the total figures, this does not mean that I have no figures at all on which to base my statement that the percentage of recognized cases referred from other departments is higher than that reported in Plymouth. I devote one comparatively short weekly out-patient session exclusively to seeing patients referred from other hospital departments. In the twelve months ending 31st December, 1964, I saw 103 recognized psychiatric cases in this clinic. In addition, I saw 54 such cases at other out-patient clinics. The consensus of opinion of our psychiatric staff, based on our routine weekly experience, is that we see, on an average, four recognized psychiatric cases per week in the general wards. These figures give an aggregate of 365 cases per year (compared with 193 in the Plymouth area) and do not even include relevant cases seen on domiciliary visits. In view of the fact that our catchment population is less than a quarter of a million (compared with one-third of a million in the Plymouth area), the percentage of cases referred is presumably higher than is the case in Plymouth—the only comparison with which I am here concerned.

MAURICE SILVERMAN.

Queen's Park Hospital,
Blackburn.

SLEEP PATTERNS IN REACTIVE AND ENDOGENOUS DEPRESSION

DEAR SIR,

Costello and Selby, in their recent paper (*Journal*, June 1965, p. 497), consider the sleep patterns in "reactive" and "endogenous" depression. They do not discuss what they mean by these terms, and thus it seems possible that their reactive depressive group may include all patients in whom depression has been precipitated by adverse circumstances. If this is the case, it is hardly surprising that their data do not agree with those of Kiloh and myself (*Journal*, July 1963, p. 451), who pointed out that many attacks of endogenous depression are so precipitated and hence used the term "neurotic" depression in preference to "reactive".

Apart, however, from any difference in definition, there is a logical error in Costello's and Selby's paper. They use non-significant results in sleep patterns to confirm the null hypothesis that "reactive and endogenous depressions do not differ in sleep pattern". Non-significant results are, of course, consistent with such a null hypothesis. But they are also consistent with the hypothesis that sleep patterns *do* differ between the groups in question. It is surprising that anyone needs to be reminded that a non-significant result does *not* confirm a null hypothesis. Such results do not confirm *any* hypothesis.

The illogical nature of Costello's and Selby's argument is well illustrated by the data they consider next. Of 28 patients with reactive depression, 21 (i.e. 75 per cent.) complained of difficulty in getting off to sleep on their first night in hospital, whereas only 7 out of 13 (54 per cent.) patients with endogenous depression made a similar claim. The between-group difference (21 per cent.) does not differ significantly from zero, but this does not therefore confirm the hypothesis that the true difference *is* zero. On the contrary, the *most probable* percentage difference between the populations from which the samples were drawn is 21 per cent. The standard error of this difference is 16 per cent. The true difference might well be zero, *but also might be considerably larger than 21 per cent.*

The same number of reactive depression patients (75 per cent.) reported that they woke up early on their first night in hospital, but 12 out of 13 (92 per cent.) with endogenous depressions made a similar complaint. Once again, the percentage difference of 17 per cent. is not significant. Nevertheless, these data are clearly consistent with the usual clinical view that endogenous depressives tend to complain of early waking more often than do reactive depressives. The data do not, therefore, confirm the null hypothesis.

The remaining data of Costello and Selby are not so strikingly at variance with their conclusions, but are still not significant. Thus their complaint that Kiloh and I applied "elaborate sophisticated statistical techniques" to perhaps unworthy data rests entirely upon a misunderstanding of the logic underlying simple tests of statistical significance.

R. F. GARSIDE.

Department of Psychological Medicine,
Queen Victoria Road,
Newcastle upon Tyne, 1.

TREATMENT OF PSYCHOGENIC DYSPAREUNIA

DEAR SIR,

I should like to congratulate Dr. Haslam (*Journal*, March 1965, p. 280) on his successful treatment of two cases of psychogenic dyspareunia by reciprocal inhibition, whilst recording a reservation about his remark that "the time taken . . . compared very favourably with any other psychiatric approach that might have been attempted."

In fact a different psychiatric approach (1) (a combination of psychotherapy and digital exploration of the vagina by women general practitioners, under psychiatric guidance) has produced very similar results; "71 patients out of 100 (81 per cent. of those with known outcome) consummated their marriages, 96 per cent. of them after 5 or fewer sessions".

To my mind, we have here a fascinating instance of how workers with different theoretical orientations may operate in rather kindred ways in the actual treatment situation, obtaining comparable results and explaining them quite differently. There would seem to be a case for investigating what it is that different psychiatric treatments have in common, instead of continuing the well-known polemic about how they differ.

R. MACKIE.

Department of Mental Health,
University of Aberdeen,
Foresterhill, Aberdeen.

REFERENCE

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KORO IN A BRITON

DEAR SIR,

To the account of a koro case mentioned by Dr. F. Bodman (*Journal*, April 1965, p. 369), I should like to add the following report of one seen informally by me during a recent visit to Britain.

The patient was a physically healthy man of 43, a book-keeper, who had never ventured beyond Western Europe. Like his father and brothers, he was of a worrying, nervous disposition, with a history of youthful stuttering. Between the ages of 15 and 24 he had indulged in masturbation, with guilt and fears of insanity. He married at 32, and although he fathered three children he remained sexually shy and took little pleasure in coitus. As a young man one of his testicles had been forced into the inguinal canal in a fall, but this was reduced. He had long been worried over and ashamed of the somewhat small size of his penis in contrast to what he held to be unduly long testes, and because of this he avoided undressing in front of others, for example in a public bath.

Since the age of 22 he had suffered three spells of depression, during which he complained of pain in the neck, back and testicles, as well as paraesthesiae in the legs. One attack coincided with his engagement. Some two months before he was seen he had become tense and depressed, with loss of libido. He was impotent, but still had occasional wet dreams. One unusually cold morning he felt his penis shrinking to about half an inch, although

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R. F. GARSIDE

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